

Discussion Draft of the Fourth Edition of the *International Code of Zoological Nomenclature*: Comments

(See also BZN 52: 228–233, 294–302; 53: 6–17)

The following are amongst the comments which have been received, all of which (whether published in the *Bulletin* or not) have been forwarded to the Editorial Committee and are being taken into full consideration. A revised Draft will be discussed at a Workshop at the International Congress of Systematic and Evolutionary Biology (ICSEB) in Budapest (17–24 August 1996).

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We would like to draw attention to a significant shift in emphasis in the Discussion Draft, relating to the precedence of 'established usage' over the identity of name-bearing types under certain circumstances (see Article 61), and to relaxation of the rules concerning the establishment and status of neotypes (Article 75). In the following comments, we have been guided by two practical considerations: first, that long-term nomenclatural stability is dependent upon stability and continuity of the existing, widely-accepted, set of rules; second, that the burden of applications to the Commission for rulings on specific cases should be as light as possible. In addition, we hold firmly to the philosophical principle that 'the fixation of the name-bearing type of a nominal taxon provides the objective standard of reference for the application of the name it bears' and 'once fixed, name-bearing types are stable and provide objective continuity in the application of names' (Article 61a). True long-term stability of nomenclature can only be achieved by rigorous maintenance of types as the objective standards of reference.

In the proposed Fourth Edition, a new section (e) has been added to the existing Article 61, to the effect that, if the identity of a name-bearing type specimen is found to conflict with usage of a widely accepted name, then existing usage is to be maintained, a replacement name-bearing type (presumably a neotype) is to be nominated, and the case is to be referred to the Commission for ratification. This contrasts with the existing Code, under which the identity of the type specimen is the ultimate arbiter of the application of the name it bears (Article 61a); in this there are no rules or recommendations covering misidentifications but the Commission can use its plenary power to change a name-bearing type, if required in the interests of stability (Article 79a).

The consequences of the proposed change will be insidious and potentially far-reaching. It reduces the incentive for confirmation of the identity of type

specimens during revisory taxonomic studies: it is sufficient merely to establish the existing usage of a name. With the passage of time, such non-rigorous practice will lead to the accumulation of errors, and when these are discovered the workload of the Commission will be increased. The concept of stability must be viewed in the long term; it should not be confused with short-term convenience. Thus, while retention of misapplied names may be convenient, the result will be a break in historical continuity, and a devaluation of the status of type specimens. Such a devaluation contradicts the very purpose of typification that is so well expressed in Article 61a. Furthermore, replacement type specimens may be separated from the material of the original author, adding to the dispersal of type material and making future comparative work still more difficult. In the new Article 61e, 'established usage' is defined as the use of a name by at least 5 different authors in at least 10 publications during the preceding 50 years. In underworked groups, or where a group has been worked by a single 'school' of taxonomists, such criteria have little meaning. In an extreme case, it could be envisaged that in an infrequently studied group a new author could dispense with the examination of historical literature and existing type specimens; the taxonomy could be entirely redone and the necessary number of publications required to establish usage of the new nomenclature could soon be achieved by a small group of researchers. While the result would undoubtedly be convenient for the workers involved, it would unquestionably be contrary to the interests of long-term stability. Poor taxonomic practice of this kind can only be discouraged by strict adherence to the rule of type specimens as unchanging standards of reference.

Similarly, we notice that changes to the rules on the establishment of neotypes have been made in the Discussion Draft. The proposed new version of Article 75j covers the rediscovery of a name-bearing type after the designation of a neotype and specifies that, if the two are considered conspecific, the neotype is to retain its status as the name-bearing type, while otherwise the case is to be referred to the Commission. In contrast, in the existing Code (Article 75h) all such cases are to be referred to the Commission, regardless of subjective assessments of taxonomic identity. The proposed new version is clearly a licence to erect neotypes without thorough search for existing types, and again devalues the status of name-bearing types by encouraging poor taxonomic practice. There is also the added element of subjectivity in the assessment of conspecificity. It is an important principle of both the existing and proposed versions of the Code that neotypes are 'to be designated only in connection with revisory work, and only in exceptional circumstances when a neotype is necessary in the interests of stability of nomenclature' (Article 75b). The new Sections 75f (previously Recommendation 75E) and 75j of Article 75 could be misused to justify the disregard of existing historical material and the creation of unnecessary neotypes. The inevitable subsequent controversies would add to the workload of the Commission.

In summary, therefore, we strongly advocate the removal of the proposed Articles 61e and 61f from the Fourth Edition of the Code, the return of Article 75f to a Recommendation, and the replacement of Article 75j by its existing wording (Article 75h in the Third Edition).

(2) Rolf G. Oberprieler

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The current Code distinguishes (Articles 45f and 45g) between those pre-1961 infraspecific (but not expressly subspecific) names originally published as 'varieties' or 'forms' and all others; in general the former are treated as subspecific, and hence available, but the latter are not. However, in the Lepidoptera and many other groups of insects (and no doubt other animals) it is abundantly clear that authors often did not use the category 'subspecies' in their descriptive taxonomy. Instead they used a loose range of terms (varieties, variations, forms, aberrations, morphs, natios, etc.) for infraspecific entities. Which term was used often depended on personal, national, linguistic or other factors rather than on clear definitions or concepts. Unless published in addition to a trinomen such names were not unambiguously established as *infrasubspecific*, but only as *infraspecific*. Many of the entities denoted by these names have long been treated as valid subspecies or even species, and to change their names on the basis of technicalities of original terminology or futile judgements of the author's concept would have grave consequences for stability.

I would like to suggest that Articles 45f and 45g be simplified so that *all* names published before 1961 as the final epithet in a trinomen are deemed to be subspecific and hence available, unless expressly stated otherwise by the author. The *availability* of names is the concern of nomenclature; their *validity* is another matter and in the realm of taxonomy.

(3) Charles D. Michener

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In general I am strongly in favor of the changes in the Discussion Draft, but I have some comments.

I am not certain that the requirement (Article 16b) that new names must be accompanied by a diagnosis in a language using the Latin alphabet is practicable. Might it not tend to cause Chinese or Russian authors (for example) to deviate from the Code, leading to multiple nomenclatures? Zoologists from such countries should be consulted as to their views of the long-term effect of the proposed wording.

The lack of gender of generic names (Article 30) is very desirable, but neither of the alternatives offered in Article 31b is satisfactory. The first is the return to the original ending of an adjectival specific epithet, but this would cause many changes and is a bad idea, contrary to the spirit of minimal changes and maximal stability characteristic of other proposed rule changes. The second is the use of the currently accepted ending until a new combination is used, at which time the ending would revert to the original; this leaves potential for changes in endings, essentially for ever. We need the following provision: After 1997, the ending of a species-group epithet is to remain as it is in an existing combination, even when the species is transferred to another nominal genus. If difference in taxonomic opinion had led to a species being placed in different genera in the five[?] years prior to 1998, such that the ending of the specific

epithet was not constant, then the ending to be accepted is that which is first in alphabetical order (e.g. the order *-a*, *-um*, *-us*).

(4) L.B. Holthuis

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I have a number of comments on the Discussion Draft.

Article 5a. I gladly concur with the (re-)introduction of the term 'epithet'; it is clear and unambiguous, whereas 'specific name' is often confused with 'species name'.

Articles 8e and 11b. It is a good idea to make *Zoological Record* a kind of 'official list' of new names, as it is already the source consulted by all zoologists wanting to find out what species have been recently described. Also, the great increase in publications and the ease with which articles can now be published makes it imperative that there be a single source for reference. The objection that the *Record* is incomplete is true, but this is mainly the fault of authors. Article 8e will be a powerful incentive to authors to notify new names, and so will greatly facilitate the work of taxonomists. However, the deadline should not be five years after the publication of the new name, but five years after the publication of the volume of *Zoological Record* dealing with the relevant year (it would be helpful if each volume of the *Record* were made to cover a calendar year).

Article 16a, e. The idea behind these provisions is sound: we have to eliminate from availability names published by authors who did not intend to describe new taxa. The botanists have done this by requiring Latin diagnoses, but the simple requirements of Article 16e are adequate. The proposal (Article 16a) that a new taxon must be compared with a named taxon of equal rank is nonsense. What about a new genus described with a single (new) species? There are no species within the genus with which the new species can be compared. The idea should be at most a Recommendation; for the Article itself the words 'characters that purport to differentiate the new nominal taxon from other taxa' are sufficient. I hope that the Code will not require a diagnosis (a term not defined in the Glossary of the present Code) as well as a description!

Article 16c, e(ii). It would be pedantic to require that the name of a new family-group nominal taxon 'must be accompanied by a statement in words designating the type genus' (Article 16c), or that an otherwise excellently defined new species-group taxon must be accompanied 'by the designation of a holotype, or syntypes' (Article 16e(ii)). The present rules have caused no difficulty at all. Why make the Code more complicated? It is bad enough as it is.

Articles 23j and 35e. I am against this limitation of adherence to priority, which I believe should be strictly followed (with the proviso that the Commission can make exceptions in special cases).

Article 29. I am against 'flexibility' in the treatment of family-group names, the derivation of which should follow correct grammar except when a change (by the Commission) is necessary to remove homonymy.

Articles 30, 31 and 32. The abandonment of gender of generic names should be made retroactive to 1758, and the original spellings of *all* names should be used

(even if grammatically incorrect or showing typographical or clerical error). This courageous step would be a great simplification of the Code.

Article 41a. This is very obscurely worded, but after reading it several times I believe that it is intended that, if a genus is based on a misidentified type genus [who is to prove that?], then the biological species involved has automatically to be taken as the type species. This is confusing taxonomy with nomenclature. The correct 'automatic' solution is to accept the nominal species fixed as type by the author; this is in accord with Article 61a(iii). If this causes confusion then the case should be referred to the Commission. If the biological species is to be taken, authors (even if they agree that there was a misidentification) may differ in their opinion about the actual identity of the material.

Article 55. These provisions for removing homonymy between family-group names are far too complicated! The present rules are adequate.

Article 74a(ii)(5). Why should it be compulsory to give a 'reason' in order for a lectotype designation to be valid? This is another unnecessary provision.

My general impressions of the changes proposed in the draft are: (1) with the laudable aim of supporting usage, vague wordings which may be interpreted differently by zoologists ('universally', 'widely', 'interested zoologists', 'substantial agreement', etc.) are proposed; (2) the draft tries to regulate too much and creates a number of quite unnecessary difficulties for zoologists working in taxonomy — instead of simplifying the Code (already too complicated) the draft will increase confusion.

(5) M. Judson

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The rules of zoological nomenclature only work because there is a consensus of opinion that they should be followed. No Code can be imposed.

As joint Editor of a largely taxonomic journal, I am worried that some may find the new proposals sufficiently radical to feel that they would no longer be bound by the Code. It would be disastrous if the community were to split into two camps, those who adhered to the new Code and those who did not. I have in mind particularly the abandonment of grammatical gender agreement, the acceptance of misspellings, and the non-availability of names not registered in the *Zoological Record* within five years.

I see few advantages in the proposed modifications, apart from the welcome tightening of rules on type designations. As they stand, the proposals often seem to run counter to their stated aims. I hope the members of the Commission will carefully consider whether they are acting in accord with the wishes of the majority.

(6) Dietrich Kadolsky

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The draft contains major departures from the present Code, and hence is likely to cause name changes. In addition to this, some of the proposals (such as the requirement that new names must appear within five years in *Zoological Record*, and

the statement that generic names have no gender) will mean that the Code will not command respect and will be taken less seriously: prevailing practice will overrule it.

I have given the Code Editorial Committee a considerable number of detailed comments on the draft. I have some particular remarks on the proposed 'Lists of Available and Potentially Valid Names' (Articles 77 and 78). It goes against common sense and the theme of all previous Codes that names should become unavailable simply by omission from a List. Mere omission, which may be due to oversight, must not have any nomenclatural effect. To delegate the compilation of such Lists to (for example) 'societies representative of international zoologists concerned with a major part of the animal kingdom' paves the way for the development of divergent nomenclatural practices in different groups of animals and possibly different parts of the world. This would thoroughly undermine the Code. I believe the status of a name should derive from a single source: this has to be the original publication and not *Zoological Record* or a 'List'.

(7) Stuart H. McKamey

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I opposed the proposal in the draft (Article 11b) that a new name would cease to be available if it had not been recorded within five years in *Zoological Record*. However, an alternative has now been put forward on the Internet and also by Ride (BZN 53: 6-7), according to which all new names would [as now] be permanently available from their original publication but a name recorded in *Zoological Record* would have precedence over a synonym so obscurely published that it was not recorded within five years.

I support such a proposal, which should extend to homonyms as well as synonyms. Perhaps a 10-year period would be more appropriate, at least initially; it could be reduced to five years (or even fewer) once systematists worldwide are accustomed to the procedure and electronic data storage and retrieval have advanced further. Thus, the alternative to Article 11b which I propose is:

Names are permanently available from their date of publication, but a name recorded in *Zoological Record* has precedence over a synonym or homonym not recorded within ten years of publication.

(8) Graham C.D. Griffiths

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The proposals in Article 31b of the draft relating to the spellings of adjectival species-group epithets would cause unnecessary changes in the usage of names. There are many epithets, at least in entomology, which were first proposed many years ago in huge aggregate genera but which have not been used in the original combination for one or even two centuries, nor are ever likely to be so used again. To revert to the original spellings in such cases would be contrary to stability and long-standing usage. Based on my considerable experience in revisionary work, especially in

Diptera, I object to both the alternatives proposed in Articles 31b and 48a. I request that a third alternative be considered: that the ending in use at the time the new Code comes into effect shall remain unchanged henceforward, both in existing and in any new combinations subsequently proposed. Please forget about original endings.

(9) Frank-Thorsten Krell

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The number of generic names has of course increased enormously since the time of Linnaeus; this is due not only to the discovery of new organisms but also to the 'splitting' of nominal genera. Apart from being the expression of taxonomic progress the splitting has had the very useful effect of permitting the same specific epithets to be used in different genera.

Even in the 18th-century some coleopterists, for example, followed Linnaeus in using only a few genera while others adopted the more refined Fabrician classification. There were no nomenclatural rules, and once a species with the epithet (say) *albus* had been removed from a genus a new species placed in the genus could be given that epithet. The identical binomina did not exist simultaneously but successively, often separated by a long interval.

Today, one or two hundred years later, the two species names with the epithet *albus* may have been in wide and stable use in different higher-rank taxa for a very long time. Nevertheless, according to Article 57b in both the draft and the current Code the junior *albus* is 'permanently invalid'. I agree with Crosskey (BZN 53: 14) that the application of the Code in these circumstances does not promote stability but is a cause of much confusion. I know of four examples of forgotten or recently discovered cases of ancient primary homonymy within the SCARABAEOIDEA (Coleoptera) but I cannot mention them, because to do so would precipitate name changes for well known species. I propose that taxonomically long-separated primary homonyms should not be replaced in future; naturally replacements already made should stand.

(10) N.Ju. Kluge

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Article 16a in the draft specifies that to be available the name of a new nominal taxon must be accompanied by a diagnosis differentiating the taxon 'from other taxa of the same rank within the next higher taxonomic category', and that these 'must be explicitly cited by name'.

This requirement is unsatisfactory and vague. For example, if two new species are described in the same paper and they are compared only one with another, are their names available? If a previously published 'comparator' name is unavailable for some technical reason, is the new name therefore also unavailable? Further objections to this provision have already been made by other commentators.

The provisions in Article 16a and 16b, taken together, make very complicated rules of availability. Suppose that an author describes and compares a new taxon with

another in a Russian text, but that the English summary omits the comparison. There could be two opinions: (1) the name is unavailable, because the comparison was not made in 'a language using the Latin alphabet'; (2) the name is available, because there is a diagnosis in such a language and the comparison ('differentiation') can be made in any script.

The expression 'language using the Latin alphabet' is unsatisfactory. Since the intention is presumably that only a few widely understood (or easily translated) languages should be permitted, these should be specified by name. It would be pleasant for me to see Russian in such a list! Many taxa have been originally described in Russian, and at least for names published before the new Code comes into effect these descriptions have to be translated. Languages apart from English, French and German are used less in the taxonomic literature than is Russian, and if the latter is to be excluded they should be also.

(11) Vitor O. Becker

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The proposition in the draft (Article 30) that genus-group names should be 'treated as words without gender, even when Greek or Latin or latinized' is ridiculous. This is for two main reasons: firstly because it is simply wrong, and secondly because the Commission must not use the Code to impose upon others who know how to do something properly that from now on they must do it wrongly. I would not feel it an obligation to submit myself to such an imposition.

Some may say 'Now with computers we have to change this rule. Otherwise information retrieval will become impossible'. This is absolutely untrue.

This matter cannot be decided by the vote of a few people. *La fuerza de la Ley* must prevail, not *La Ley de la Fuerza*.

(12) S. Endrödy-Younga (and eight colleagues)

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Every scientific or technical discipline has its established form of communication in terms of language, chemical or mathematical formulae or rules of diagram structure. The retention of the internationally accepted method of communication is essential for general understanding in the subject.

The rules of communication in biological systematics and the formation of scientific names of taxa are fixed in the International Codes of nomenclature, and the

chosen language for names is Latin. All living languages are subject to changes due to general usage, but with Latin this is not the case.

The proposed changes to remove the grammar from Latin are not appropriate. Such changes would bring the language of communication to the level of the lowest common denominator and make a joke of the [still recommended] Latin. No taxonomist is expected to be proficient in Latin; what is required is a knowledge of the few rules about the construction of sound binomina and the naming of higher taxonomic categories. Advice on these is not difficult to obtain. The problems resulting from the degradation of Latin are reflected by the number of emended rules and recommendations put forward in an effort to make sense of the proposed changes. However, these rules and often contradictory recommendations would only serve to cause further confusion and impede stability in nomenclature.

On a separate subject, we fully support the Code Editorial Committee in upholding *Zoological Record* as an irreplaceable source of information in biological systematics. The reiterated duty of an author to ensure the notification of a publication supports the work of the editorial department of this most important publication.